
Psychology and Anomalous Observations

The Question of ESP in Dreams

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ABSTRACT: *Books by psychologists purporting to offer critical reviews of research in parapsychology do not use the scientific standards of discourse prevalent in psychology. Experiments at Maimonides Medical Center on possible extrasensory perception (ESP) in dreams are used to illustrate this point. The experiments have received little or no mention in some reviews to which they are clearly pertinent. In others, they have been so severely distorted as to give an entirely erroneous impression of how they were conducted. Insofar as psychologists are guided by these reviews, they are prevented from gaining accurate information about research that, as surveys show, would be of wide interest to psychologists as well as to others.*

In recent years, evidence has been accumulating for the occurrence of such anomalies as telepathy and psychokinesis, but the evidence is not totally convincing. The evidence has come largely from experiments by psychologists who have devoted their careers mainly to studying these anomalies, but members of other disciplines, including engineering and physics, have also taken part. Some psychologists not primarily concerned with parapsychology have taken time out from other professional concerns to explore such anomalies for themselves. Of these, some have joined in the experimentation (e.g., Crandall & Hite, 1983; Lowry, 1981; Radin, 1982). Some have critically reviewed portions of the evidence (e.g., Akers, 1984; Hyman, 1985). Some, doubting that the phenomena could be real, have explored nonrational processes that might encourage belief in their reality (e.g., Ayeroff & Abelson, 1976). Still others, considering the evidence substantial enough to justify a constructive theoretical effort, have struggled to relate the apparent anomalies to better established knowledge in a way that will render them less anomalous (e.g., Irwin, 1979) or not anomalous at all (e.g., Blackmore, 1984). These psychologists differ widely in their surmise about whether the apparent anomalies in question will eventually be judged real or illusory; but they appear to agree that the evidence to date warrants serious consideration.

Serious consideration of apparent anomalies seems an essential part of the procedures of science,

regardless of whether it leads to an understanding of new discoveries or to an understanding of how persuasive illusions arise. Apparent anomalies—just like the more numerous observations that are not anomalous—can receive appropriate attention only as they become accurately known to the scientists to whose work they are relevant. Much parapsychological research is barred from being seriously considered because it is either neglected or misrepresented in writings by some psychologists—among them, some who have placed themselves in a prime position to mediate interaction between parapsychological research and the general body of psychological knowledge. In this article, I illustrate this important general point with a particular case, that of experimental research on possible ESP in dreams. It is a case of especially great interest but is not unrepresentative of how psychological publications have treated similar anomalies.

The Maimonides Research

The experimental evidence suggesting that dreams may actually be influenced by ESP comes almost entirely from a research program carried out at the Maimonides Medical Center in Brooklyn, New York. Among scientists active in parapsychology, this program is widely known and greatly respected. It has had a major indirect influence on the recent course of parapsychological research, although the great expense of dream-laboratory work has prevented it from being a direct model.

None of the Maimonides research was published in the journals that are the conventional media for psychology. (The only possible exception is that a summary of one study [Honorton, Krippner, & Ullman, 1972] appeared in convention proceedings of the American Psychological Association.) Much of it was published in the specialized journals of parapsychology. The rest was published in psychiatric or other medical journals, where it would not be noticed by many psychologists. Most of it was summarized in popularized form in a book (Ullman, Krippner, & Vaughan, 1973) in which two of the researchers were joined by a popular writer whose own writings are clearly not in the scientific tradition, and the book departs from the pattern of scientific reporting that characterizes the original research reports.

How, then, would this research come to the attention of psychologists, so that its findings or its errors might in time be evaluated for their significance to the body of systematic observations upon which psychology has been and will be built? The experiments at Maimonides were published between about 1966 and 1972. In the years since—now over a decade—five books have been published by academic psychologists that purport to offer a scholarly review and evaluation of parapsychological research. They vary in the extent to which they seem addressed to psychologists themselves or to their students, but they seem to be the principal route by which either present or future psychologists, unless they have an already established interest strong enough to lead them to search out the original publications, might become acquainted with the experiments on ESP in dreams. I propose to review how these five books have presented knowledge about the experiments. First, however, I must offer a summary of the experiments; without that, my review would make sense only to readers already well acquainted with them.

The experiments at Maimonides grew out of Montague Ullman's observations, in his psychiatric practice, of apparent telepathy underlying the content of some dreams reported by his patients—observations parallel to those reported by many other psychiatrists. He sought to determine whether this apparent phenomenon would appear in a sleep laboratory under controlled conditions that would seem to exclude interpretations other than that of ESP. He was joined in this research by psychologist Stanley Krippner, now at the Saybrook Institute in San Francisco, and a little later by Charles Honorton, now head of the Psychophysical Research Laboratories in Princeton, New Jersey. Encouraged by early findings but seeking to improve experimental controls and identify optimal conditions, these researchers, assisted by numerous helpers and consultants, tried out various modifications of procedure. No one simple description of procedure, therefore, can be accurate for all of the experiments. But the brief description that follows is not, I believe, misleading as an account of what was generally done.

The Experimental Procedure

A subject would come to the laboratory to spend the night there as would-be percipient in a study of possible telepathic influence on dreams. He or she met and talked with the person who was going to serve as agent (that is, the person who would try to send a telepathic message), as well as with the two experimenters taking part that night, and procedures were

explained in detail unless the percipient was a repeater for whom that step was not necessary. When ready to go to bed, the percipient was wired up in the usual way for monitoring of brain waves and eye movements, and he or she had no further contact with the agent or agent's experimenter until after the session was completed. The experimenter in the next room monitored the percipient's sleep and at the beginning of each period of rapid eye movements (REM), when it was reasonably certain the sleeper would be dreaming, notified the agent by pressing a buzzer.

The agent was in a remote room in the building, provided with a target picture (and sometimes accessory material echoing the theme of the picture) randomly chosen from a pool of potential targets as the message to be concentrated on. The procedure for random choice of a target from the pool was designed to prevent anyone else from knowing the identity of the target. The agent did not open the packet containing the target until isolated for the night (except for the one-way buzzer communication). Whenever signaled that the percipient had entered a REM period, the agent was to concentrate on the target, with the aim of communicating it telepathically to the percipient and thus influencing the dream the percipient was having. The percipient was oriented toward trying to receive this message. But of course if clairvoyance and telepathy are both possible, the percipient might have used the former—that is, might have been picking up information directly from the target picture, without the mediation of the agent's thoughts or efforts. For this reason, the term *general extrasensory perception (GESP)* would be used today, though the researchers more often used the term *telepathy*.

Toward the approximate end of each REM period, the percipient was awakened (by intercom) by the monitoring experimenter and described any dream just experienced (with prodding and questioning, if necessary, though the percipient of course knew in advance what to do on each awakening). At the end of the night's sleep, the percipient was interviewed and was asked for impressions about what the target might have been. (The interview was of course double-blind; neither percipient nor interviewer knew the identity of the target.) The dream descriptions and morning impressions and associations were recorded and later transcribed.

The original research reports and the popular book both present a number of very striking similarities between passages in the dream transcripts and the picture that happened to be the night's target. These similarities merit attention, yet they should in themselves yield no sense of conviction. Perhaps any transcript of a night's dreaming contains passages of striking similarity to any picture to which they might be compared. The Maimonides research, however, consisted of carefully planned experiments designed

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to permit evaluation of this hypothesis of random similarity, and I must now turn to that aspect.

Results

To evaluate the chance hypothesis, the researchers obtained judgments of similarity between the dream content and the actual target for the night, and at the same time obtained judgments of similarity between the dream content and each of the other potential targets in the pool from which the target had been selected at random. The person judging, of course, had no information about which picture had been randomly selected as target; the entire pool (in duplicate) was presented together, with no clue as to which picture had been the target and which ones had not. That is, in the experimental condition a picture was randomly selected from a pool and concentrated on by the agent, and in the control condition a picture was left behind in the pool. Any consistent difference between target and nontarget in similarity to dream content, exceeding what could reasonably be ascribed to chance, was considered an apparent anomaly.

The data available for the largest number of sessions came from judgments made by judges who had no contact with the experiment except to receive (by mail, generally) the material necessary for judging (transcripts of dreams and interview and a copy of the target pool). For many sessions, judgments were also available from the dreamer; he or she, of course, made judgments only after completing participation in the experiment as dreamer (except in some series where a separate target pool was used for each night and the dreamer's judgments could be made at the end of the session). For many sessions, judgments were made for the dream transcripts alone and for the total transcript including the morning interview; for consistency I have used the latter, because it involved judges who had more nearly the same information as the subjects.

The only form in which the data are available for all series of sessions is a count of hits and misses. If the actual target was ranked in the upper half of the target pool, for similarity to the dreams and interview, the outcome was considered a hit. If the actual target was ranked in the lower half of the pool, the outcome was considered a miss. The hit-or-miss score is presented separately in Table 1 for judges and for subjects in the first two data columns. Where information is not supplied for one or the other, the reason is generally that it was impossible for the researchers to obtain it, and for a similar reason the number of cases sometimes varies.¹

Each data row in Table 1 refers to one segment of the research, and segments for the most part are labeled as they were in the table of Ullman et al. (1973, pp. 275-277). Segments that followed the general procedure I described—all-night sessions, with an agent concentrating on the target during each of the percipient's REM periods—are gathered together in the first eight lines, A through H (in five of these segments, all but A, C, and H; a single percipient continued throughout a series, and in four of these the percipient was a psychologist). Other types of segments are presented in the rest of the table. Lines I, J, and K summarize precognitive sessions; here the target was not selected until after the dreaming and interview had been completed. The target consisted of a set of stimuli to be presented directly to the percipient after it had been selected in the morning. Lines L and M represent GESP sessions in which the percipient's dreams were monitored and recorded throughout the night, but the agent was attempting to transmit only before the percipient went to sleep or just after, or sporadically. Line N refers to a few clairvoyance sessions; these were like the standard GESP sessions except that there was no agent (no one knew the identity of the target). Finally, Line O reports on some GESP sessions in which each dream was considered separately; these formed a single experiment with four percipients, comparing nights involving a different target for each REM period with nights involving repeated use of a single target.

Regardless of the type of session (considering the five types I have described), each session fell into one of two categories: (a) pilot sessions, in which either a new dreamer or a new procedure was being tried out; these appear in lines H, K, and N, or (b) sessions in an experimental series, planned in advance as one or more sessions for each of two or more subjects, or as a number of sessions with the same dreamer throughout. Most of the researchers' publications were devoted to the results obtained in the experimental series, but the results of the pilot sessions have also been briefly reported.

A glance at the score columns for judges and for subjects is sufficient to indicate a strong tendency for an excess of hits over misses. If we average the outcome for judges and for subjects, we find that hits exceed misses on every one of the 15 independent lines on which outcome for hits and misses differs. (On Line E hits and misses occur with equal frequency.) By a simple sign-test, this outcome would be significant beyond the 0.0001 level. I would not stress the exact value here, for several reasons. There was no advance

judgments. In a few of the pilot sessions (Lines H, K, and N) only the subject's judgment was sought, and in some sessions only that of one or more judges; in a few the mean judges' rating was neither a hit nor a miss but exactly at the middle.

¹ Of course, usable judgments could not be obtained from the subject in precognitive sessions, because at the time of judging he or she would already know what the target had been. For Line F, the single subject was unable to give the extra time required for judging, and for Line O one of the four subjects failed to make

Table 1
Summary of Maimonides Results on Tendency for Dreams to Be Judged More Like Target Than Like Nontargets in Target Pool

Series	Judges' score		Subjects' score		z or t resulting from judgments		Sources
	Hit	Miss	Hit	Miss	Judges	Subjects	
GESP: Dreams monitored and recorded throughout night; agent "transmitting" during each REM period							
A. 1st screening	7	5	10	2	$z = 0.71^b$	$z = 1.33^b$	Ullman, Krippner, & Feldstein (1966)
B. 1st Erwin	5	2	6	1	$z = 2.53^b$	$z = 1.90^b$	Ullman et al. (1966)
C. 2nd screening	4	8	9	3	$z = -.25^b$	$z = 1.17^b$	Ullman (1969)
D. Posin	6	2	6	2	$z = 1.05^c$	$z = 1.05^c$	Ullman (1969)
E. Grayeb	3	5	5	3	$z = -.63^c$	$z = 0.63^c$	Ullman, Krippner, & Vaughan (1973)
F. 2nd Erwin	8	0			$t = 4.93^a$		Ullman & Krippner (1969)
G. Van de Castle	6	2	8	0	$t = 2.81^a$	$t = 2.74^a$	Krippner & Ullman (1970)
H. Pilot sessions	53	14	42	22	$z = 4.20^b$	$z = 2.21^b$	Ullman et al. (1973)
Precognition: Dreams monitored and recorded throughout night; target experience next day							
I. 1st Bessent	7	1			$t = 2.81^a$		Krippner, Ullman, & Honorton (1971)
J. 2nd Bessent	7	1			$t = 2.27^a$		Krippner, Honorton, & Ullman (1972)
K. Pilot sessions	2	0			$z = 0.67^c$		Ullman et al. (1973)
GESP: Dreams monitored and recorded throughout night; agent active only at beginning or sporadically							
L. Sensory bombardment	8	0	4	4	$z = 3.11^b$	$z = 0.00^c$	Krippner, Honorton, Ullman, Masters, & Houston (1971)
M. Grateful Dead	7	5	8	4	$z = 0.61^c$	$z = 0.81^c$	Krippner, Honorton, & Ullman (1973)
Clairvoyance: Dreams monitored and recorded throughout night; concealed target known to no one							
N. Pilot sessions	5	3	4	5	$z = 0.98^b$	$z = 0.00^b$	Ullman et al. (1973)
GESP: Single dreams							
O. Vaughan, Harris, Parise	105	98	74	79	$z = 0.63^c$	$z = -.32^c$	Honorton, Krippner, & Ullman (1972)

Note. GESP = general extrasensory perception. Italics identify results obtained with procedures that preserve independence of judgments in a series. For some series, the published source does not use the uniform measures entered in this table, and mimeographed laboratory reports were also consulted. Superscripts indicate which measure was available, in order of priority.

^a Ratings. ^b Rankings. ^c Score (count of hits and misses).

plan to merge the outcomes for judges and subjects. Moreover, the various series could be split up in other ways. Although I think my organization of the table is very reasonable (and I did not notice this outcome until after the table was constructed), it is not the organization selected by Ullman et al. (1973); their table, if evaluated statistically in this same way, would not yield so striking a result. What is clear is that the tendency toward hits rather than misses cannot rea-

sonably be ascribed to chance. There is some systematic—that is, nonrandom—source of anomalous resemblance of dreams to target.

Despite its breadth, this "hitting" tendency seems to vary greatly in strength. The data on single dreams—Line O—suggest no consistency. At the other extreme, some separate lines of the table look impressive. I will next consider how we may legitimately evaluate the relative statistical significance of

separate parts of the data on all-night sessions. (I will not try to take exact account here of the fact that the single-dream data are not significant, though it is wise to have in mind that the exact values I cite must be viewed as slightly exaggerated, in the absence of any explicit advance prediction that the results for all-night sessions and for single dreams would differ greatly.)

Two difficulties, one general and one specific, stand in the way of making as thorough an evaluation as I would wish. The general difficulty is that the researchers turned the task of statistical evaluation over to various consultants—for the most part, different consultants at various times—and some of the consultants must also have influenced the choice of procedures and measures. The consultants, and presumably the researchers themselves, seem not to have been at that time very experienced in working with some of the design problems posed by this research nor in planning how the research could be done to permit effective analysis. Much of the research was not properly analyzed at the time, and for much of it the full original data are no longer available. (The researchers have been very helpful in supplying me with material they have been able to locate despite dispersal and storage of the laboratory's files. Perhaps additional details may be recovered in the future.) The result is that completely satisfactory analysis is at present possible only for some portions of the data.

The specific difficulty results from a feature of the research design employed in most of the experimental series, a feature whose implications the researchers did not fully appreciate at the time. If a judge is presented with a set of transcripts and a set of targets and is asked to judge similarity of each target to each transcript, the various judgments may not be completely independent. If one transcript is so closely similar to a particular target that the judge is confident of having recognized a correct match, the judge (or percipient, of course) may minimize the similarity of that target to the transcripts judged later. Instructions to judges explicitly urged them to avoid this error, but we cannot tell how thoroughly this directive was followed. Nonindependence would create no bias toward either positive or negative evidence of correspondence between targets and transcripts, but it would alter variability and thus render inappropriate some standard tests of significance. I have entered in the two succeeding columns of the table a t or a z that can be used in evaluating the statistical significance of the departure from chance expectancy (t is required when ratings are available, and z must be used when only rankings or score counts are available, because sample variability in the former case is estimated from the data but in the latter case must be based conservatively on a theoretical distribution.) If ratings were available, they were used; if not, rankings were used if available; otherwise, score count was used.

Is there likely to have been much of this nonindependence in the series where it was possible? A pertinent fact is that the hits were not generally direct hits. That is, there was no overwhelming tendency for the correct target to be given first place rather than just being ranked in the upper half of the target pool. This greatly reduces the strength of the argument that ordinary significance tests are grossly inaccurate because of nonindependence. Because certainty is not possible, however, we need to separate results according to whether the procedures permitted this kind of nonindependence. In the table, I have italicized results that cannot have been influenced by this difficulty (either because each night's ratings were made by a different person or because each night in a series had, and was judged in relation to, a separate target pool) or that closely approximate this ideal condition.

The outcome is clear. Several segments of the data, considered separately, yield significant evidence that dreams (and associations to them) tended to resemble the picture chosen randomly as target more than they resembled other pictures in the pool. In the case of evaluation by outside judges, two of the three segments that are free of the problem of nonindependence yield separately significant results: The pilot sessions (Line H) yield a z of 4.20, and thus a p of .00002. An experiment with distant but multisensory targets (Line L) yields a z of 3.11 and a p of .001. If we consider segments in which judgments may not be completely independent of each other and analyze them in the standard way, we find that the two series with psychologist William Erwin as dreamer are also significant (if nonindependence of judgments does not seriously interfere), Line B with a z of 2.53 ($p < .01$) and Line F with a t of 4.93 and 7 df ($p < .01$). The two precognitive series (Lines I and J), each with 7 df , yield t s of 2.81 and 2.27, with p values slightly above and below .05, respectively.

Segment results based on the subjects' own judgments of similarity are less significant than those based on judgments by outside judges. Only two segments reach minimal levels of statistical significance: Line G, where the t of 2.74 with 7 df is significant at the .05 level, and Line H, where the z of 2.21 is significant at the .05 level.

The statistical evaluation of the separate segments of the Maimonides experiments also permits a more adequate evaluation of their overall statistical significance. For judgments by outside judges, three segments are free of the potential nonindependence of successive judgments (Lines H, L, and N). Putting these three together by the procedure Mosteller and Bush (1954, pp. 329–330) ascribed to Stouffer (recommended by Rosenthal [1984, p. 72] as the "simplest and most versatile" of the possible procedures), the joint p value is $< .000002$. For the subjects' own judgments, six segments are available (Lines A, C, G, H,

L, and N), and their joint p value is less than .002. The other segments of the data have the problem of potential nonindependence of successive judgments, and even if the exaggeration of significance may be small for a single line, I would not want to risk compounding it in an overall p . Their prevailing unity of direction, however (direction not being subject to influence by the kind of nonindependence involved here), and the substantial size of some of the differences, justify the inference that the overall evidence of consistency far exceeds that indicated by only those selected segments for which a precise statistical statement is possible. The impression given by the mere count of hits and misses is thus fully confirmed when more sensitive measures are used.

Parapsychological experiments are sometimes criticized on the grounds that what evidence they provide for ESP indicates at most some very small effects detectable only by amassing large bodies of data. Those to whom this criticism has any appeal should be aware that the Maimonides experiments are clearly exempt from it. The significant results on Lines F and G of the table, for example, are each attributable basically to just eight data points.

If replications elsewhere should eventually confirm the statistically significant outcome of the Maimonides experiments, would the fact of statistical significance in itself establish the presence of the kind of anomaly called ESP? Of course not. Statistical significance indicates only the presence of consistency and does not identify its source. ESP, or the more general term *psi*, is a label for consistencies that have no identifiable source and that suggest transfer of information by channels not familiar to present scientific knowledge. A judgment about the appropriateness of the label, and thus about the "ESP hypothesis," is complex. It depends on a variety of other judgments and knowledge—how confidently other possible sources of the consistent effect can be excluded, whether other lines of experimentation are yielding results that suggest the same judgment, and so on.

I believe many psychologists would, like myself, consider the ESP hypothesis to merit serious consideration and continued research if they read the Maimonides reports for themselves and if they familiarized themselves with other recent and older lines of experimentation (e.g., Jahn, 1982, and many of the chapters in Wolman, 1977).

Some parapsychological researchers—among them the Maimonides group—have written at times as though a finding of statistical significance sufficiently justified a conclusion that the apparent anomaly should be classified as ESP. I can understand their choice of words, which is based on their own confidence that their experiments permitted exclusion of other interpretations. But perhaps psychologists who in the future become involved in this area may prefer

to use a term such as *anomalies*, so as to avoid variable and possibly confusing connotations about the origin of the anomalies. Zusne and Jones (1982) wisely prepared the way for this usage in speaking of *anomalous psychology*. But meanwhile, psychologists need not cut themselves off from knowledge of relevant facts because of dissatisfaction with the terminology surrounding their presentation.

Attempted Replications Elsewhere

The Maimonides pattern of controlled experiment in a sleep laboratory, obviously, is extremely time consuming and expensive, and replication seems to have been attempted so far at only two other sleep laboratories. At the University of Wyoming, two experiments yielded results approximately at mean chance expectation—slightly below in one study (Belvedere & Foulkes, 1971), slightly above in the other (Foulkes et al., 1972). In a replication at the Boston University School of Medicine (Globus, Knapp, Skinner, & Healey, 1968), overall results were not significantly positive, though in this instance encouragement for further exploration was reported. The researchers had decided in advance to base their conclusions on exact hits—that is, placing the target first, rather than just in the upper half; by this measure, the results were encouraging, though not statistically significant. Moreover, to quote the researchers, "*Post hoc* analysis revealed that the judges were significantly more correct when they were more 'confident' in their judgments. . . . Further conservatively designed research does seem indicated because of these findings" (Globus et al., 1968, p. 365).

A study by Calvin Hall (1967) is sometimes cited as a replication that confirmed the Maimonides findings; in truth, however, although it provided impressive case material, it was not done in a way that permits evaluation as a replication of the Maimonides experiments. Several small-scale studies, done without the facilities of a sleep laboratory, have been reported that are not replications of even one of the more ambitious Maimonides experiments but each of which reports positive results that might encourage further exploration (Braud, 1977; Child, Kanthamani, & Sweeney, 1977; Rechtschaffen, 1970; Strauch, 1970; Van de Castle, 1971). In the case of these minor studies—unlike the Maimonides studies and the three systematic replications—one must recognize the likelihood of selective publication on the basis of interesting results. Taken all together, these diverse and generally small-scale studies done elsewhere do, in my opinion, add something to the conviction the Maimonides experiments might inspire, that dream research is a promising technique for experimental study of the ESP question.

The lack of significant results in the three systematic replications is hardly conclusive evidence

against eventual replicability. In the Maimonides series, likewise, three successive replications (Lines C, D, and E in Table 1) yielded no significant result, yet they are part of a program yielding highly significant overall results.

If results of such potentially great interest and scientific importance as those of the Maimonides program had been reported on a more conventional topic, one might expect them to be widely and accurately described in reviews of the field to which they were relevant, and to be analyzed carefully as a basis for sound evaluation of whether replication and extension of the research were indicated, or of whether errors could be detected and understood. What has happened in this instance of anomalous research findings?

Representation of the Maimonides Research in Books by Psychologists

It is appropriate to begin with E. M. Hansel's 1980 revision of his earlier critical book on parapsychology. As part of his attempt to bring the earlier book up to date, he included an entire chapter on experiments on telepathy in dreams. One page was devoted to a description of the basic method used in the Maimonides experiments; one paragraph summarized the impressive outcome of 10 of the experiments. The rest of the chapter was devoted mainly to a specific account of the experiment in which psychologist Robert Van de Castle was the subject (the outcome is summarized in Line G of my Table 1) and to the attempted replication at the University of Wyoming (Belvedere & Foulkes, 1971), in which Van de Castle was again the subject. Another page was devoted to another of the Maimonides experiments that was also repeated at the University of Wyoming (Foulkes et al., 1972). Hansel did not mention the replication by Globus et al. (1968), whose authors felt that the results encouraged further exploration. Hansel gave more weight to the two negative outcomes at Wyoming than to the sum of the Maimonides research, arguing that sensory cues supposedly permitted by the procedures at Maimonides, not possible because of greater care taken by the Wyoming experimenters, were responsible for the difference in results. He did not provide, of course, the full account of procedures presented in the original Maimonides reports that might persuade many readers that Hansel's interpretation is far from compelling. Nor did he consider why some of the other experiments at Maimonides, not obviously distinguished in the care with which they were done from the two that were replicated (e.g., those on Lines E, M, and O of Table 1) yielded a close-to-chance outcome such as Hansel might have expected sensory cuing to prevent.

Hansel exaggerated the opportunities for sensory cuing—that is, for the percipient to obtain by ordinary sensory means some information about the target for

the night. He did this notably by misinterpreting an ambiguous statement in the Maimonides reports, not mentioning that his interpretation was incompatible with other passages; his interpretation was in fact erroneous, as shown by Akers (1984, pp. 128–129). Furthermore, Hansel did not alert the reader to the great care exerted by the researchers to eliminate possible sources of sensory cuing. Most important is the fact that Hansel did not provide any plausible account—other than fraud—of how the opportunities for sensory cuing that he claimed existed would be likely to lead to the striking findings of the research. For example, he seemed to consider important the fact that at Maimonides the agent could leave his or her room during the night to go to the bathroom, whereas in Wyoming the agent had a room with its own bathroom, and the outer door to the room was sealed with tape to prevent the agent from emerging. Hansel did not attempt to say how the agent's visit to the bathroom could have altered the details of the percipient's dreams each night in a manner distinctively appropriate to that night's target. The only plausible route of influence on the dream record seems to be deliberate fraud involving the researchers and their subjects. The great number and variety of personnel in these studies—experimenters, agents, percipients, and judges—makes fraud especially unlikely as an explanation of the positive findings; but Hansel did not mention this important fact.

It appears to me that all of Hansel's criticisms of the Maimonides experiments are relevant only on the hypothesis of fraud (except for the mistaken criticism I have mentioned above). He said that unintentional communication was more likely but provided no evidence either that it occurred or that such communication—in any form in which it might have occurred—could have produced such consistent results as emerged from the Maimonides experiments. I infer that Hansel was merely avoiding making explicit his unsupported accusations of fraud. Fraud is an interpretation always important to keep in mind, and it is one that could not be entirely excluded even by precautions going beyond those used in the Wyoming studies. But the fact that fraud was as always, theoretically possible hardly justifies dismissal of a series of carefully conducted studies that offer important suggestions for opening up a new line of inquiry into a topic potentially of great significance. Especially regrettable is Hansel's description of various supposed defects in the experiments as though they mark the experiments as being carelessly conducted by general scientific criteria, whereas in fact the supposed defects are relevant only if one assumes fraud. A reader who is introduced to the Maimonides research by Hansel's chapter is likely to get a totally erroneous impression of the care taken by the experimenters to avoid various possible sources of error. The one thing they could

not avoid was obtaining results that Hansel considered a priori impossible, hence evidence of fraud; but Hansel was not entirely frank about his reasoning.

An incidental point worth noting is that Hansel did not himself apply, in his critical attack, the standards of evidence he demanded of the researchers. His conclusions were based implicitly on the assumption that the difference of outcome between the Maimonides and the Wyoming experiments was a genuine difference, not attributable to random variation. He did not even raise the question, as he surely would have if, in some parallel instance, the Maimonides researchers had claimed or implied statistical significance where it was questionable. In fact, the difference of outcome might well have arisen from random error; for the percipient's own judgments the difference is significant at the 5% level (2-tailed), but for the outsiders' judgments it does not approach significance.

Another 1980 book is *The Psychology of Transcendence*, by Andrew Neher, in which almost 100 pages are devoted to "psychic experience." Neher differed from the other authors I refer to in describing the Maimonides work as a "series of studies of great interest" (p. 145), but this evaluation seems to be negated by his devoting only three lines to it and four lines to unsuccessful replications.

A third 1980 publication, *The Psychology of the Psychic*, by David Marks and Richard Kammann, provides less of a general review of recent parapsychology than Hansel's book or even Neher's one long chapter. It is largely devoted to the techniques of mentalists (that is, conjurors specializing in psychological rather than physical effects) and can be useful to anyone encountering a mentalist who pretends to be "psychic." Most readers are not likely to be aware that parapsychological research receives only limited attention. The jacket blurbs give a very different view of the book, as do the authors in their introductory sentences:

ESP is just around the next corner. When you get there, it is just around the next corner. Having now turned over one hundred of these corners, we decided to call it quits and report our findings for public review. (Marks & Kammann, 1980, p. 4)

Given this introduction to the nature of the book, readers might suppose it would at least mention any corner that many parapsychologists have judged to be an impressive turning. But the Maimonides dream experiments received no mention at all.

Another volume, by psychologist James Alcock (1981), quite clearly purports to include a general review and evaluation of parapsychological research. Alcock mentioned (p. 6) that Hansel had examined the Maimonides experiments, but the only account of them that Alcock offered (on p. 163) was incidental to a discussion of control groups. By implication he

seemed to reject the Maimonides experiments because they included no control groups. He wrote that "a control group, for which no sender or no target was used, would appear essential" (p. 163). Later he added, "One could, alternatively, 'send' when the subject was not in the dream state, and compare 'success' in this case with success in dream state trials" (p. 163). The first of these statements suggests a relevant use of control groups but errs in calling it essential; in other psychological research, Alcock would have doubtless readily recognized that within-subject control can, where feasible, be much more efficient and pertinent than a separate control group. His second statement suggests a type of experiment that is probably impossible (because in satisfactory form it seems to require the subject to dream whether awake or asleep and not to know whether he or she was awake or asleep). This second kind of experiment, moreover, has special pertinence only to a comparison between dreaming and waking, not to the question of whether ESP is manifested in dreaming.

Alcock, in short, did not seem to recognize that the design of the Maimonides experiments was based on controls exactly parallel to those used by innumerable psychologists in other research with similar logical structure (and even implied, curiously enough, in his own second suggestion). He encouraged readers to think that the Maimonides studies are beyond the pale of acceptable experimental design, whereas in fact they are fine examples of appropriate use of within-subject control rather than between-subjects control.

The quality of thinking with which Alcock confronted the Maimonides research appeared also in a passage that did not refer to it by name. Referring to an article published in *The Humanist* by Ethel Grodzins Romm, he wrote,

Romm (1977) argued that a fundamental problem with both the dream telepathy research and the remote viewing tests is that the reports suffer from what she called "shoe-fitting" language; she cited a study in which the sender was installed in a room draped in white fabric and had ice cubes poured down his back. A receiver who reported "white" was immediately judged to have made a "hit" by an independent panel. Yet, as she observed, words such as "miserable", "wet", or "icy" would have been better hits. . . . Again, the obvious need is for a control group. Why are they not used? (p. 163)

What Romm described as "shoe fitting" (misinterpreting events to fit one's expectations) is an important kind of error that is repeatedly made in interpretation of everyday occurrences by people who believe they are psychic. But the dream telepathy research at Maimonides was well protected against this kind of error by the painstaking controls that Alcock seemed not to have noticed. Surely Romm must be referring to some other and very sloppy dream research?

Not at all. The details in this paragraph, and even more in Romm's article, point unmistakably, though inaccurately, to the fifth night of the first pre-cognitive series at Maimonides. The actual details of target and response would alone deprive it of much of its value as an example of shoe fitting. As reported by Krippner, Ullman, & Honorton (1971), the target was a morning experience that included being in a room that was draped with white sheets. The subject's first dream report had included the statement, "I was just standing in a room, surrounded by white. Every imaginable thing in that room was white" (p. 201). There is more similarity here than Romm and Alcock acknowledged in mentioning from this passage only the single word "white."

More important, however, is the fact that the experiment they were referring to provided no opportunity for shoe fitting. The procedures followed in the experiment were completely misrepresented in a way that created the illusion that the possibility existed. There was no panel, in the sense of a group of people gathered together and capable of influencing each other. The judges, operating independently, separately judged every one of the 64 possible combinations of target and transcript yielded by the eight nights of the experiment, not just the eight correct pairings, and they had no clues to which those eight were. Their responses are hardly likely to have been immediate, as they required reading the entire night's transcript. Because each judge was working alone and was not recording times, there would have been no record if a particular response had been immediate, and no record of what particular element in the transcript led to an immediate response.

I looked up in a 1977 issue of *The Humanist* the article by Romm that Alcock cited. The half page on shoe-fitting language gave as examples this item from the Maimonides research and also the SRI remote-viewing experiments (Puthoff & Targ, 1976) done at SRI International. In both cases what was said was pure fiction, based on failure to note what was done in the experiments and in particular that the experimenters were well aware of the danger of shoe-fitting language and that the design of their experiments incorporated procedures to ensure that it could not occur. Romm's ignorance about the Maimonides research and her apparent willingness to fabricate falsehoods about it should be recognized by anyone who had read any of the Maimonides research publications. Yet Alcock accepted and repeated the fictions as though they were true. His presentation in the context of a book apparently in the scientific tradition seems to me more dangerous than Romm's original article, for anyone with a scientific orientation should be able to recognize Romm's article as propaganda. Its title, for example, is "When You Give a Closet Occultist a PhD, What Kind of Research Can You

Expect?" and it repeatedly speaks of "cult phuds," meaning people with PhDs who are interested in parapsychological problems. Alcock's repetition of Romm's misstatements in a context lacking these clues may well be taken by many a reader as scholarly writing based on correct information and rational thought. Paradoxically, both Alcock's paragraph and Romm's article are excellent examples of the shoe-fitting error that both decry in others who are in fact carefully avoiding it.

The last of the five books that bring, or fail to bring, the Maimonides research to the attention of psychologists and their students is *Anomalistic Psychology: A Study of Extraordinary Phenomena of Behavior and Experience*, a 1982 volume by Leonard Zusne and Warren H. Jones. This is in many ways an excellent book, and it is also the one of the five that comes closest to including a general review of important recent research in parapsychology. Its brief account of the Maimonides dream experiments, however, misrepresented them in ways that should seriously reduce a reader's interest in considering them further.

Zusne and Jones's description of the basic procedure made three serious errors. First, it implied that one of the experimenters had a chance to know the identity of the target. ("After the subject falls asleep, an art reproduction is selected from a large collection randomly, placed in an envelope, and given to the agent" p. 260). In fact, precautions were taken to ensure that no one but the agent could know the identity of the target. Second, the authors stated that "three judges . . . rate their confidence that the dream content matches the target picture" (p. 260), leading the reader to suppose that the judges were informed of the identity of the target at the time of rating. In fact, a judge was presented with a dream transcript and a pool of potential targets and was asked to rate the degree of similarity between the transcript and each member of the pool, while being unaware of which member had been the target. Third, there was a similarly, though more obscurely, misleading description of how ratings were obtained from the dreamer.

This misinformation was followed by even more serious misrepresentation of the research and, by implication, of the competence of the researchers. Zusne and Jones (1982) wrote that Ullman and Krippner (1978) had found that dreamers were not influenced telepathically unless they knew in advance that an attempt would be made to influence them. This led, they wrote, to the subject's being "primed prior to going to sleep" through the experimenter's

preparing the receiver through experiences that were related to the content of the picture to be telepathically transmitted during the night. Thus, when the picture was Van Gogh's *Corridor of the St. Paul Hospital*, which depicts a lonely figure in the hallways of a mental hospital, the receiver: (1)

heard Rosza's *Spellbound* played on a phonograph; (2) heard the monitor laugh hysterically in the room; (3) was addressed as "Mr. Van Gogh" by the monitor; (4) was shown paintings done by mental patients; (5) was given a pill and a glass of water; and (6) was daubed with a piece of cotton dipped in acetone. The receiver was an English "sensitive," but it is obvious that no psychic sensitivity was required to figure out the general content of the picture and to produce an appropriate report, whether any dreams were actually seen or not. (pp. 260-261)

If researchers were to report positive results of the experiment described here by Zusne and Jones and were to claim that it provided some positive evidence of ESP, what would a reader conclude? Surely, that the researchers were completely incompetent, but probably not that they were dishonest. For dishonesty to take such a frank and transparent form is hardly credible.

Incompetence of the researchers is not, however, a proper inference. The simple fact, which anyone can easily verify, is that the account Zusne and Jones gave of the experiment is grossly inaccurate. What Zusne and Jones have done is to describe (for one specific night of the experiment) some of the stimuli provided to the dreamer the next morning, *after* his dreams had been recorded and his night's sleep was over. Zusne and Jones erroneously stated that these stimuli were provided *before* the night's sleep, to prime the subject to have or falsely report having the desired kind of dream. The correct sequence of events was quite clearly stated in the brief reference Zusne and Jones cited (Ullman & Krippner, 1978), as well as in the original research report (Krippner, Honorton, & Ullman, 1972).

I can understand and sympathize with Zusne and Jones's error. The experiment they cited is one in which the nocturnal dreamer was seeking to dream in response to a set of stimuli to be created and presented to him the next morning. As may be seen in Table 1, results from such precognitive sessions (all done with a single subject) were especially strong. This apparent transcendence of time as well as space makes the precognitive findings seem at least doubly impossible to most of us. An easy misreading, therefore, on initially scanning the research report, would be to suppose the stimuli to have been presented partly in advance (because some parts obviously involved a waking subject) and partly during sleep.

This erroneous reading on which Zusne and Jones based their account could easily have been corrected by a more careful rereading. In dealing with other topics, they might have realized the improbability that researchers could have been so grossly incompetent and could have checked the accuracy of their statements before publishing them. Zusne and Jones are not alone in this tendency to quick misperception of parapsychological research through pre-

conception and prejudice; we have already seen it in Alcock's book. Alcock (1983) wrote the review of Zusne and Jones's book for *Contemporary Psychology*, the book-review journal of the American Psychological Association, and he did not mention this egregious error, even though very slight acquaintance with the Maimonides research should suffice to detect it.

Discussion

The experiments at the Maimonides Medical Center on the possibility of ESP in dreams clearly merit careful attention from psychologists who, for whatever reason, are interested in the question of ESP. To firm believers in the impossibility of ESP, they pose a challenge to skill in detecting experimental flaws or to the understanding of other sources of error. To those who can conceive that ESP might be possible, they convey suggestions about some of the conditions influencing its appearance or absence and about techniques for investigating it.

This attention is not likely to be given by psychologists whose knowledge about the experiments comes from the books by their fellow psychologists that purport to review parapsychological research. Some of those books engage in nearly incredible falsification of the facts about the experiments; others simply neglect them. I believe it is fair to say that none of these books has correctly identified any defect in the Maimonides experiments other than ones relevant only to the hypothesis of fraud or on inappropriate statistical reasoning (easily remedied by new calculations from the published data). I do not mean that the Maimonides experiments are models of design and execution. I have already called attention to a design flaw that prevents sensitive analysis of some of the experiments; and the control procedures were violated at one session, as Akers (1984) pointed out on the basis of the full information supplied in the original report. (Neither of these genuine defects was mentioned in any of the five books I have reviewed here, an indication of their authors' general lack of correct information about the Maimonides experiments.)

Readers who doubt that the falsification is as extreme as I have pictured it need only consult the sources I have referred to. Their doubt might also be reduced by familiarity with some of James Bradley's research (1981, 1984). In his 1984 article, he reported similar misrepresentations of fact on a topic, robustness of procedures of statistical inference, on which psychologists would not be thought to have nearly the strength of preconception that many are known to have about ESP. How much more likely, then, falsification on so emotionally laden a topic as ESP is for many psychologists! In the earlier article, Bradley (1981) presented experimental evidence (for college students, in this case, not psychologists) that confi-

dence in the correctness of one's own erroneous opinions is positively correlated with the degree of expertise one believes oneself to have in the field of knowledge within which the erroneous opinion falls. This finding may help in understanding why the authors of some of these books did not find it necessary to consider critically their own erroneous statements.

A very considerable proportion of psychologists have a potential interest in the question of ESP. In a recent survey (Wagner & Monnet, 1979) of university professors in various fields, 34% of psychologists were found to consider ESP either an established fact or a likely possibility, exactly the same proportion as considered it an impossibility. In this survey, psychologists less frequently expressed a positive opinion than did members of other disciplines, a finding that may be attributable to psychologists' better understanding of sources of error in human judgment. There seems to be no equally sound reason for the curious fact that psychologists differed overwhelmingly from others in their tendency to consider ESP an impossibility. Of natural scientists, only 3% checked that opinion; of the 166 professors in other social sciences, not a single one did.

Both of these groups of psychologists have been ill served by the apparently scholarly books that seem to convey information about the dream experiments. The same may be said about some other lines of parapsychological research. Interested readers might well consult the original sources and form their own judgments.

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all involved, especially the public, who benefit from the work of both disciplines.

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The Natural Alliance of Psychology and Nursing: Substance as Well as Practice

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DeLeon, Kjervik, Kraut, and VandenBos (November 1985) described the "natural alliance" of psychology and nursing, centering around "defining the appropriate scope of practice" in each discipline. Although their discussion did an admirable job of illuminating the historical parallels between clinical psychologists and clinical nurse specialists (such as nurse practitioners), it leaves the impression that these legal and policy issues are the only links between the two disciplines.

In our experience, there is also a "natural alliance" between psychology and nursing based on subject matter. Nursing is concerned with human responses, some of which are psychological, to states of illness and wellness. Research in nursing science is concerned with describing and predicting those human responses and with testing interventions that alter them. It goes without saying that psychology is involved in similar ways with human responses of many kinds, including those related to health.

This alliance between nursing and psychology is probably most obvious in the subareas of psychosocial nursing and health psychology. Research and practice in psychosocial nursing center on the care of clients with mental and emotional illnesses. Consequently, both scholars and clinicians working in this subarea have

much in common with professionals in other disciplines who share the same clientele and concerns. Health psychology, on the other hand, serves clients whose illnesses include somatic aspects. The research and practice priorities of health psychologists, then, often overlap with those of other health professionals, including nurses.

What may be less obvious is that other specialty areas in nursing and psychology also have considerable overlap. As one example, consider parent and child nursing, a specialty area including the traditional concerns of maternity and pediatric care. At the University of Washington in Seattle, the Parent and Child Nursing Department includes, among its 41 faculty members, 9 with doctoral degrees in psychology or educational psychology, 2 with interdisciplinary doctoral degrees in which psychology played a key part, and 2 with degrees in nursing science whose dissertations were psychological in nature. These 13 faculty members range in rank from research associate to full professor; some have regular academic appointments, and others serve on the research faculty; some, but not all, are registered nurses. Together, they bring to their work graduate training in clinical, counseling, developmental, social, experimental, cognitive, and physiological psychology, as well as expertise in research methods, design, and measurement. All are involved to some degree in the research activities of the Nursing School, recently reported to be number one in the country (Chamings, 1984). These research projects range widely in content and scope and include such topics as home intervention in high-risk families with newborns, parent education to reduce disruptive behavior in preschoolers, provision of effective education for children with chronic illnesses and their parents, and improvement in the care of infants in neonatal intensive care units. Many of these faculty are associated with the University's Child Development and Mental Retardation Center, and six of them are also members of the MacArthur Foundation Research Network on the Transition from Infancy to Early Childhood. Both of these affiliations represent substantial investments in interdisciplinary work that includes not only nursing and psychology but also pediatrics, social work, psychiatry, nutrition, and other allied disciplines.

Although our department and school may be somewhat unique in the extent of collaboration between nursing and psychology, we believe that the "natural alliance" is a strong one, and we expect to be joined in these collaborative ventures

by other psychologists and nurses around the country.

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Further Implications of Anomalous Observations for Scientific Psychology

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Recently, Child (November 1985) pointed out that many anomalous observations within psychological (specifically, parapsychological) research are barred from consideration by the mainstream of psychology through both neglect and misrepresentations due to the philosophical prejudice of reviewers. Specifically, he described research conducted at the Maimonides Medical Center on possible telepathic information transfer during dreaming. This study appears to have been well designed, with results that are extremely compelling. Even for those who reject the explanation of extrasensory perception (ESP) out of hand, the results represent intriguing data that need to be accounted for. As Child pointed out, serious consideration of apparent anomalies seems an essential part of the procedure of science, and it is important that accurate information about such anomalous findings be disseminated within the fold of psychologists. Often, however, scientific psychologists have difficulty keeping on top of the literature within even a narrow domain of interest and never venture beyond the boundaries of their immediate research. However, certain potentially revolutionary findings that have implications for the most fundamental tenets of our world view, such as the results reported on by Child, are relevant to and should be considered by all scientists. This is particularly true in the case of some recent anomalous experimental findings in the area of quantum physics (Aspect, Dalibard, & Roger, 1982) that have great implications for all of the natural sciences, including psychology.

The dominant philosophical views within psychology and the other natural sciences can be characterized as realism and physicalistic monism. Realism is the view that external reality exists and has

definite properties independent of any act of observation. Physicalistic monism is the view that the processes commonly referred to as "mind" or "consciousness" are totally reducible to underlying physical processes.

A third fundamental assumption that has exerted a strong influence on the world view of science could be termed the *locality assumption* (or Einstein separability), which holds that events can have only local influence, with the maximum boundary constrained by the speed of light. (It is this assumption specifically that makes many of the findings of parapsychological research "anomalous.") These three assumptions about reality have formed the basis for the current scientific paradigm. The results of the Aspect experiment (as well as the Maimonides research) seem to call for a reevaluation of some of these above-mentioned assumptions.

Local realistic theories of physical reality place a limit on the extent to which distant events can be correlated, whereas quantum mechanics allows this limit to be exceeded. Those physicists who felt uncomfortable with the paradoxes that abound in the predictions of quantum mechanics, such as the seeming connection of particles at a distance, felt that these relationships would be explained by some as-yet-undiscovered supplementary parameters or "hidden variables" that represent the actual underlying reality—the fundamental clockwork—below the unreal world of the quantum.

The Aspect et al. (1982) experiment was designed to test directly the predictions of "hidden variable" theories versus the predictions of quantum mechanics regarding the behavior of photons scattered in opposite directions from a source. Streams of photon pairs scattered in opposite directions from the same source were passed through polarizers whose settings were varied randomly and observed by two detectors that measured the polarization of the split pairs. According to quantum theory, this property of polarization does not exist until it is measured. Classical realistic theories such as the hidden variable explanation hold that each photon has a "real" polarization from the moment it is created. Because the photon pairs are emitted simultaneously, their polarizations are originally correlated, but quantum theory and hidden variable theories make different predictions about the nature of the final correlations after the photons pass through the polarizers and the detectors. In particular, hidden variable (local realistic) theories predict that the final correlations between the photon

pairs will obey a relation called the Bell inequality (for a detailed discussion, see d'Espagnat, 1979), whereas quantum theory predicts a violation of the inequality. In essence, quantum theory predicts that measuring the polarization of one photon in a pair "causes" (this term is used loosely) a change in the polarization of the second to bring it into correspondence, even though the two are far enough apart that a causal signal would have to propagate faster than the speed of light in order to connect them.

Since 1972, five of seven experimental tests of the Bell inequality have supported the predictions of quantum theory. However, none of these earlier experiments was a rigorous test of Einstein separability (i.e., that no signal can propagate faster than the speed of light), because the settings of the polarizing and detecting instruments were determined well in advance, and a possible argument could be made that the setting of one of the instruments might conceivably affect events observed at the other (this influence would not have had to propagate faster than light), or the settings of the instruments could have modified hidden parameters at the source of the photon pairs.

The major improvement in the Aspect et al. experiment involved adding rapid switching devices that changed the settings of the instruments while the photons were in flight. Each polarizer was replaced by a setup involving a switching device followed by two polarizers with different orientations. The two switches were driven at different frequencies, making them function in an uncorrelated way. Switching occurred about each 10 nanoseconds, compared to the photon transit time of about 20 nanoseconds. It was thus impossible for any information about the experimental setup to travel from one part of the apparatus to the other and affect the outcome of any measurement unless such an influence was exceeding the speed of light.

The results of the Aspect et al. experiment support the predictions of quantum mechanics. When the polarization of a photon was changed, the second photon was changed in the same manner. In other words, the photons started out with the same polarity and were found to still have the same polarity after each passed through an independent device that shifted its polarity to one of two possible states at random. These results violate the Bell inequality (which limits the correlations of the photons based on the considerations of set theory) by five standard deviations. Thus, this more rigorous procedure violated the inequality to a greater extent than

any of the previous tests. Local realistic (hidden variable) theories therefore appear to be untenable, and at least one of the premises underlying those theories of reality (i.e., realism or Einstein separability) must be in error.

Now invoking another basic assumption of science—that it is legitimate to draw general conclusions from consistent observations or experiments—we must consider the macroscopic implications if the picture of reality that quantum theory gives us is valid. This has implications for measurement and logic, two of the fundamental epistemological tools of science, in terms of their ability to answer ultimate questions about our existence.

With the emphasis on measurement, the world is reduced to quantities and the relationships between them. There is a fundamental belief that the quantitative description of things is paramount and even complete in itself. It is as if we have given measurement ontological significance and confused quantification with explanation. Despite their utility, and even necessity in most scientific endeavors, operational definitions lack any kind of ultimate meaning and therefore are not satisfactory for achieving a final understanding of the world. Because they are so much a part of our ordering schema of reality, we forget the arbitrariness of our units of division and measurement, including even the most basic units of extension and duration. There is a culture whose basic temporal unit is the time it takes a pot of rice to boil. Our units of nanoseconds are certainly more sophisticated and accurate but no less arbitrary. These divisions (such as seconds and centimeters) are not a part of objective reality but are part of the cognitive framework that we have created in order to organize that reality.

Quantum paradoxes point to the limitations of logic and measurement for even the understanding of basic levels of physical reality. Logical absolutes (e.g., something cannot be both A and not-A simultaneously) break down at the quantum level. What is termed the "problem of measurement," exemplified by the Heisenberg uncertainty principle, tends to puncture our image as uninvolved observers as we measure so-called "objective" reality.

Of course the ultimate limitations of logic are in the areas of self-reference and completeness—a logical system is not capable of explaining itself, and for any sufficiently powerful logical system, there will be truths not expressible as theorems of that system. There is thus a good possibility that we may have to transcend logic as the sole criterion if we are to achieve

levels of ultimate ontological understanding.

We must recognize that measurement implies consciousness, and not rule out the possibility of consciousness playing a primary role in even our understanding of physical reality. The relationship of consciousness to measurement is involved in our basic conceptualization of "number" and "quantity." Number not only implies comparison (which ultimately involves subjective criterion) but also seems to be intimately connected to such troublesome (anomalous?) concepts as infinity and the cardinality of the continuum.

One very basic conceptual pitfall can be illustrated by the way in which we conceptualize infinity. Infinity is usually thought of in terms of vastness or a very large number, when of course the concept has nothing to do with "largeness" or even "number." It is not a point on the number line, but a different order of things entirely (I am as "close" to infinity at one as I am at one million). Yet infinity seems to underlie the number line and to be implied by the existence of quantity. A better conceptualization of infinity would be as undivided wholeness.

This concept of undivided wholeness may lead to the key in solving many of the anomalies implied by quantum theory or posed by the results of parapsychological research. To explain the type of phenomena we have been considering in the experiments mentioned above, several theorists have proposed that an object (e.g., a photon or a dreaming subject) is an abstraction from some underlying wholeness, thus reversing the usual conceptualization of the relationship between part and whole. This underlying wholeness would represent a different order of reality that is unmanifest in nature. The underlying reality is termed the *implicate* (or *enfolded*) order by Bohm (1973). Space, time, and matter represent the *explicate* (or *unfolded*) order. Bohm stated:

One is led to a new notion of unbroken wholeness which denies the classical idea of analyzability of the world into separately and independently existing parts. . . . We have reversed the usual classical notion that the independent 'elementary' parts of the world are the fundamental reality, and that the various systems are merely particular contingent forms and arrangements of these parts. Rather, we say that inseparable quantum interconnectedness of the whole universe is the fundamental reality, and that relatively independently behaving parts are merely particular and contingent forms within this whole. (Bohm & Hiley, 1975, pp. 96, 102)

Thus, in the case of space-like separated photons (or dreaming subjects and isolated "senders"), the connectivity might not be based on some transference of energy, but

on the underlying whole from which both are abstracted. It remains a paradox only as long as we are bound to our classical realistic notions of space, time, and discreteness.

What meaning does all this have for psychology as a science? The issue here concerns metaphor and vision, assumptions and values. Although there have been several proposals by brain researchers that single quantum exchanges may be significantly involved at the synaptic junction (see, e.g., Eccles, 1953; Walker, 1970), most psychologists and philosophers of psychology have tended to either ignore completely or downplay the relevance of quantum theory for psychology. This is especially true in the case of anomalous implications of quantum theory, such as the effects of the observer on the observation (implied by the Heisenberg uncertainty principle), because such effects were thought to be negligible on macrostructures. For example, Feigl (1975) stated:

The influence of observation on observed objects maintained by a majority of present-day physicists is in any case negligible in regard to macro-objects. It does not hurt the moon to look at it, even if electrons get a 'kick' out of being looked at. (p. 21)

But this type of argument is based on the naive classical assumption that quantum uncertainty is based on some disturbance of a system during the measurement process. This, however, is not the case. The uncertainty exists in the nature of reality itself. According to the fundamental equation of quantum mechanics, there is no such thing as an electron that possesses both a precise momentum and a precise position. Bohr (1958), one of the founders of quantum mechanics, stated: "In quantum mechanics, we are not dealing with an arbitrary renunciation of more detailed analysis of atomic phenomena, but with a recognition that such an analysis is *in principle* (italics his) excluded" (p. 62). The implications of the experiments based on Bell's inequality have given indications that many of the paradoxes of quantum mechanics extend into the world of macroscopic events as well (see Clauser, 1976; Freedman & Clauser, 1972; Fry & Thompson, 1976; Laméhi-Rachti & Mittag, 1976). In the words of Stapp (1971),

The important thing about Bell's theorem is that it puts the dilemma posed by quantum phenomena clearly into the realm of macroscopic events. . . . (It) shows that our ordinary ideas about the world are somehow profoundly deficient even on the macroscopic level. (p. 1303)

The Aspect et al. (1982) experiment in particular should serve as a caveat to shake us out of our complacency regarding

some of our most cherished and implicit assumptions about the nature of reality. Concepts that have been fundamental to the description of the physical world for centuries, such as discreteness, causality, time, space, and number, have come into question during the last 70 or 80 years. The model of reality emerging today presents a fundamentally different picture than did the old classical notions of discrete, locally connected particles arranged in absolute space and moving through absolute time. Particle is being replaced by process, causality by synchronicity, and discreteness by undivided wholeness. We in psychology cannot afford to ignore these fundamental changes in assumptions as we attempt to plumb the depths of human consciousness.

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Secondary Sources in Parapsychological Research: A Vicious Cycle

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Having taught courses in experimental parapsychology on an annual basis since 1980, and having published several articles on the topic of computerized testing of extrasensory perception (ESP; Vitulli, 1982, 1983; Vitulli, Cain, & Broome, 1985), I was elated to read Child's (November 1985) effort to "turn psychologists around" with respect to prejudice toward psi research in general and prejudice toward research into the possibility of ESP in dreams in particular.

The secondary sources that surveyed research on ESP and other anomalous experiences referred to by Child (e.g., Hansel, 1980; Zusne & Jones, 1982) are surely not unique in kind to the field of parapsychology. Textbooks in general psychology, experimental psychology, personality, and social psychology, for example, tend to slant material toward the author's bent—behavioral, physiological, cognitive—at best by selecting studies according to meet space limitations or at worst by blatantly misleading the reader with biased interpretations of the primary research.

Fortunately, there is a correction factor associated with these more conventional areas of psychology. Most psychologists are specialists in one or more of these subject matters. Therefore, they are more likely to dig into the primary sources in order to satisfy their own professional curiosity. This is not necessarily so with respect to parapsychology. Few psychologists specialize in this currently esoteric field.

The likelihood that a psychologist who reads one of the secondary sources (survey texts in ESP) will go on to read the primary sources (journal articles) is low. Two important reasons for this reluctance are (a) journals in parapsychology are not always available in college libraries, and (b) the reader may have already been prejudiced by the misleading interpretations contained in the secondary source. Thus, the vicious cycle continues.

Let us reinforce Child's (1985) plea for more attention to the primary sources of research in parapsychology. Perhaps we may break that vicious cycle.

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Not so Anomalous Observations Question ESP in Dreams

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The *American Psychologist* is the best public vehicle for effectively reaching a majority of psychologists with diverse interests. So Child's (1985) critique of reviews critical to parapsychology illustrated on the positive the openness of *AP* reviewers and editors to provide a forum for Child, who felt that the Maimonides dream research had not come to the attention of many psychologists. The need for the critical reviews that Child attacked, however, is also illustrated. On the negative, the article bears several marks of pseudoscience (Radner & Radner, 1982), and in fact, is an excellent case study in the psychology of belief (Alcock, 1981; Singer & Benassi, 1981).

Child pleaded that the "procedures of science" require a serious consideration of the Maimonides research by psychologists, who (to their credit!), in contrast to scientists in all other disciplines, tend to consider ESP an impossibility (Wagner & Monnet, 1979). What Child proposed as an experimental hypothesis is that ESP influences dreams, or *general extrasensory perception* (GESP) may operate "without the mediation of an agent's thoughts or efforts" (p. 1220), or *Psi*, having "no identifiable source," transfers information "by channels not familiar to present scientific knowledge" (p. 1224). Some immeasurable influence by an unknown means with or without human agents supposedly results in some increment of knowledge that does not allow for any ordinary (non-anomalous) explanation. This is not a simple problem of "terminology." Rather, with no definable mechanisms in nature

or in normal science for ESP, GESP, or *Psi*, most scientists would stop without going further.

Child, however, asserted that the parapsychology critics have not been scientific. With reference to research by Bradley (1981, 1984), he suggested in *ad hominem* attacks that parapsychology critics, who are not primarily parapsychologists, have wrapped themselves in their "erroneous opinions" with the corresponding intensity of their belief in themselves as experts (p. 1229). C. E. M. Hansel's (1980) parsimonious explanations for the Maimonides research by sensory cueing and fraud were respectively labeled as "exaggerated" and "not entirely frank" (pp. 1225-1226). Alcock (1981, 1983) and Zusne and Jones (1982) were linked in an apparent conspiracy of "preconception and prejudice" (p. 1228). Those authors received the brunt of Child's attack. Although Hansel's (1980) criticisms have been available for many years, Neher (1980) and Marks and Kammann (1980) were also criticized by Child for not devoting space to the Maimonides research. Romm (1977) was attacked for her "ignorance" and "apparent willingness to fabricate falsehoods" (p. 1227). Instead, Child proposed that his analysis merits attention along with that of Akers (1984), both of whom are committed to the ESP hypothesis by belief and research.

Child asserted that familiarization with the Maimonides reports and other lines of experimentation would support the serious consideration of the ESP hypothesis. He asked psychologists to accept research that was "not properly analyzed at the time" and for which "the full original data are no longer available" (p. 1223). Consequently, we cannot inspect the data for "experimental flaws" or "other sources of error" as Child challenged (p. 1228). At another point, he called us to accept statistically significant results "attributable basically to just eight data points" (p. 1224). The opportunities for chance or deliberate fraud involving so few data points is a clear danger. The nearest replications of the Maimonides research somehow (by their number?) "add something" to the "promising technique" displayed by the Maimonides research—despite "the lack of significant results in the three systematic replications" (p. 1224). How many times do we have to accept the null hypothesis before rejecting the experimental ESP hypothesis? Further, these confirmations of the null hypothesis do not necessarily support "eventual replicability" as Child expected (p. 1225).

Indeed, there are striking parallels of the Maimonides research to other parapsy-

chological studies. The comparisons, however, would lead one to regard the Maimonides research with caution. Child deemphasized the statistical excess of hits over misses ($p < .0001$), but he appealed to the fact that "there is some systematic—that is, nonrandom—source of anomalous resemblance of dreams to targets" (p. 1222). The source, however, need not be anomalous. In the Pearce-Pratt experiments (Rhine & Pratt, 1954), the excess of hits over misses was described by a bimodal distribution (Hansel, 1980, p. 114; Zusne & Jones, 1982, p. 378). The non-random distribution suggested a non-anomalous human intervention, most likely fraud.

In the Maimonides research, judges evaluated "every one of the 64 possible combinations of target and transcript" (p. 1227). However, there is also an obvious similarity of matching dreams to targets in the Maimonides research to studies of remote viewing (Targ & Puthoff, 1974, 1977). The identification of scenes by "remote viewing" is explained more parsimoniously by cues in the transcripts of described scenes presented to judges, and by the subject's subjective validation in post-hoc confirmatory visits of target sites (Marks & Kammann, 1980, pp. 12-41).

Uri Geller, instead of matching dreams to targets, matched his drawings to his "perception" of drawings inside envelopes. Geller's success is easily accounted for by ordinary peeking and cueing (Marks & Kammann, 1980, pp. 105-107; Randi, 1982b, pp. 39-60). The fact that in the Maimonides research there were a "great number and variety of personnel" does not make fraud "especially unlikely" as Child asserted, but to the contrary it increased the potential for fraud by only one or another of the participants—"experimenters, agents, percipients, and judges" (p. 1225). Psychologists, perhaps, more than others—except professional magicians (Randi, 1982a)—ought to be sensitive to issues of experimental control and bias in studies involving human subjects.

The Maimonides research need not be cloaked in anomaly, any more than unusual animal behaviors need be cloaked in "cognition" or "insight" rather than empirically validated principles of learning (Epstein, Kirshnit, Lanza, & Rubin, 1984; Terrace, 1979). Anomalous findings need not have mysterious sources—only perhaps less appealing sources than those found in normal science. The best scientific assessment of parapsychological research is still to be found in critical reviews (Hofstadter, 1982; Kurtz, 1985). Since the time of William James, the scientific case for parapsychology has not been convinc-

ing. Why should researchers devote any more time to such research?

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Reply to Clemmer

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Clemmer (this issue, pp. 1173-1174) gives no indication that he is disturbed at the false accounts that Alcock (1981), Marks and Kammann (1980), and Romm (1977) have given of the Maimonides research, nor even that he has looked at the original research reports to check whether I was right that the accounts by those authors are false in extremely important ways. He seems to rely on a priori knowledge that there can be no anomalies and to be glad that psychologists are more likely than other scientists to adhere to this a priori knowledge, regardless of where this reliance leads them.

Clemmer's interpretation of the Maimonides results is based on analogies to defects he claims in three other pieces of parapsychological research. The bimodality he asserts for the Pearce-Pratt experiments seems to have no striking parallel in the Maimonides experiments. The defects he asserts to have characterized certain "remote viewing" and clairvoyance experiments are of types against which the Maimonides experiments were well protected by the experimental procedures. Thus Clemmer seems to follow in the tradition established by Alcock (1981) and Zusne and Jones (1982). Though he differs from them in not offering explicitly a false account of the dream experiments, the misrepresentations he implies are almost as bad.

The scientific tradition is complex and diverse. My respect for the role of general theory is a main reason that my commitment "to the ESP hypothesis" (p. 1173) does not go beyond a belief that the hypothesis merits serious exploration in hope of future discovery of the processes underlying the apparent anomalies. But facts are also important, and devotion to a theory to the point of disregarding or altering pertinent facts seems hardly compatible with the scientific tradition. A broad contribution of ESP experiments to psychology may come from their misrepresentation, if awareness of it alerts psychologists to the danger that excessive reliance on general theory may place our discipline at a disadvantage in the development of strikingly new areas of knowledge.

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Unethical Intimacy: Don't Blame Students

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In an attempt to identify factors in the unacceptable yet persistent occurrence of sexual intimacy between educators and students, Glaser and Thorpe (January 1986) seem to have concluded that it is the student's responsibility to recognize the unethical nature of the sexual contact. This conclusion is bolstered by the authors' suggestions that (a) some students enter into intimate relationships with professors because they are lonely or have experienced some type of disruption in their personal lives, and (b) many students consent to these "dual relationships."

The notion that personal disruptive experiences in students' lives, specifically divorce or separation, lead to intimacy with professors seems totally unwarranted. Glaser and Thorpe admitted that their correlational data were inadequate for establishing either the sequence of events (they did not ask whether a change in marital status came before or after the sexual contact) or the motivation for the relationship (they did not ask about emotional states). However, implicit in their suggestion is an assumption that an emotional need on the part of the student offers an opportunity or even encouragement for the professor's initiation of sexual contact. This assumption puts the responsibility on the student and is tantamount to blaming the victim for the sexual contact. It should be noted that whether or not the student is "more open to dual relationships" as Glaser and Thorpe (1986) suggested (p. 48), data clearly indicate that the educator, and not the student, typically initiates the sexual contact (Robinson & Reid, 1985). This finding means that the

student is in the uncomfortable position of having to reject his or her superior.

In their discussion of the students' judgments of coercion in the relationship, Glaser and Thorpe included a comparison of judgments made at the time of the sexual contact and judgments made at the time of the survey. Because only 28% of the respondents who experienced sexual contact perceived the use of coercion at the time, the authors concluded that the remaining respondents participated willingly in the sexual relationships. The notion that a lack of coercion means consent is another assumption that appears to focus the responsibility on the student. Past studies indicate that victims of lesser status are more vulnerable, and such relationships should be considered exploitative (Bouhoutsos, Holroyd, Lerman, Forer, & Greenberg, 1983; Pope, Levenson, & Schover, 1979). Although Glaser and Thorpe (1986) did consider the possibility that even "consenting" involvement may be exploitative, they still cautioned "against any uniform characterizations of these relationships" (p. 49).

Glaser and Thorpe's assumptions, which suggest that students' emotional needs trigger sexual involvement and which accept the absence of coercion as consent, are based on a failure to recognize the essentially unequal status of the participants in these intimate relationships. Psychologists, acting in their roles as educators, must admit that sexual intimacies with students represent more than simply a dual relationship between consenting adults. Whether the power inequities are made explicit or remain implicit, they exist. Psychologists who engage their students in sexual relationships should be cognizant of the fact that as authority figures with both superior expertise and legitimate control, they must accept culpability for the experience. It is time for the APA to develop clearer guidelines to protect from sexual exploitation the students who place their confidence and trust in their professors, supervisors, and advisers.

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Some Comments on a Revolution in the Training of Professional Psychologists

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There were several aspects of the article by Fox, Kovacs, and Graham (September 1985) upon which I would like to comment briefly.

I agree with the authors on some of their stated principles but not on others. For example, the setting up of a two-year professional training program for psychologists appears worthwhile. There are many diagnostic and therapeutic skills that can be learned by bright and motivated individuals as attested to by the pioneering project of Rioch and her colleagues in the 1960s (Rioch, 1967; Rioch, Elkes, & Flint, 1965). Furthermore, the studies that have attempted to compare the relative efficacy of professional and subprofessional mental health workers have not demonstrated conclusively the superiority of the former (Durlak, 1979, 1981; Hattie, Sharpley, & Rogers, 1984; Nietzel & Fisher, 1981). The use of subdoctoral psychologists should also tend to lessen the cost of psychological services, a worthwhile social goal.

I also agree very strongly with their rejection of the freestanding professional school as a desirable model for training professional psychologists, although I was surprised that these authors made this explicit recommendation. One of them, Kovacs, has been identified with the development of the freestanding professional school, and I would have been interested in the reasons for this change in viewpoint. On the other hand, I view the training of professional psychologists *solely* in university schools of psychology with an exclusive emphasis on professional psychology as potentially short sighted. Separating professional training from scientific and research training is fraught with potential dangers for both professional and research training. A profession that attempts this kind of isolation from research and scientific interaction would appear to be short sighted. Large numbers of students certainly can be graduated from such programs, but the development of the field will suffer. What I believe would be a better goal is the creation of truly comprehensive schools of psychology where both basic